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be broken by a plane; and so on. Concerning the psychology of space, this is explained in the usual manner, emphasis being placed on the motor adjustments.

The treatment of the physical sciences is rather cursory, as one might expect from the amount of space devoted to it. The empirical and tentative character of our various laws is shown, so M. Poincaré states, in the changing values and new interpretations necessary for the various principles, those, among others, of Carnot, Newton, Lavoisier, Mayer, etc.

The latter part of the book (Part III.) is taken up with a criticism of a certain M. le Roy, while at the end of the book, science is defined as 'a system of relations' (p. 266). Science does not lead us to know the true *nature* of things, but rather the true *relations* of things (p. 266).

I have attempted to touch upon only certain more or less interesting parts of the book in a suggestive manner. It requires close reading and rereading to get all there is out of it. And the large field which it tries to cover presupposes a varied and extensive knowledge at the start.

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Esquisse d'une théorie biologique du sommeil. E. CLAPARÈDE. *Archives de Psychologie.* Tome IV., 1905. Pp. 245–349.

The various physiological theories of sleep are first reviewed, some of which are classified by the author as 'hypotheses of mechanism,' others as 'autonomous theories.' Under the former head falls the widely accepted theory of *cerebral anæmia,* well known from the writings of Mosso, Howell and others; likewise the directly opposed theory of *cerebral hyperæmia.* In the same class, also, is placed Duval's ingenious hypothesis of the *retraction of the neurones,* according to which sleep and other dis-associative mental states have for their cause a severance of continuity in the neural paths, resulting from a retraction of the dendritic processes. Against these and some other views which he cites, the author offers the following general criticisms:

1. The supposed facts upon which the theories are based are not sufficiently well grounded, and the testimony of various writers is in some cases quite contradictory, as witness the uncertainty whether the brain is in a condition of anæmia or hyperæmia during sleep. Recent histological studies (*e. g.,* those of Apâthy, to which no allusion is made) have likewise discredited the retraction hypothesis.[1]

2. These phenomena, which are adduced as *causes,* might just as well be regarded as the *effects* of sleep.

3. Even supposing these phenomena to be in reality the immediate causes of sleep, we are not told *why,* for example, these changes in the circulation occur, or *why* the retraction of the neurones, etc.

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[1] Temporary *functional* interruptions in the continuity of nerve paths must, however, be assumed, almost of necessity; and such functional interruptions would, of course, furnish as good a physiological basis for sleep as the temporary *organic* discontinuity of Duval's hypothesis.

The '*autonomous*' theories, on the other hand, 'regard fatigue, *i. e.*, the wear and tear of the tissues produced by the activity of waking life, as the efficient cause of sleep; sleep is itself considered as a phase of organic regeneration; it is, then, in its turn, the condition of the state of waking which succeeds it.' Such theories are thus self-sufficient, or 'autonomous,' so far as they go. There are two classes of hypotheses here to be noted, one of which assumes the exhaustion of certain substances necessary to the normal life of the organism, these being restored during sleep; the other assumes a condition of *auto-intoxication* (*e. g.*, the 'autonarcose carbonique' of R. Dubois).

To the hypotheses of this class Claparède offers a truly formidable list of objections, in presenting which I have not followed strictly the author's own mode of arrangement. (1) It is not justifiable to confuse various artificial states of stupefaction with sleep proper. (2) May not the increase of CO_2 in the blood (shown by Dubois to occur in hibernating animals) be the *effect* rather than the *cause* of sleep? (3) There is no parallelism between the degree of exhaustion and the amount of sleep which ensues, as witness the case of insomnia. (4) The alternation of sleeping and waking ought to conform to a periodicity of much briefer phases. At the moment of going to sleep, our processes of destructive metabolism have, *ex hypothese,* reached a point *just sufficient* to produce this effect. Why, then, does sleep last so long? The process of recuperation (or of elimination) should bring us to the waking level in a very short time. Thus the rhythm should be more like that of respiration, the beat of the heart, etc. (5) The toxic conception is anti-physiological. We experience none of the baneful effects of narcotization; on the contrary, there is a trophic, or reparative, action during sleep. (6) Interest, volition and suggestion play an important part in hastening or retarding sleep. (7) How explain the effects of darkness, monotonous stimuli, etc.? (8) We may go to sleep almost instantly, and display a complete mental lucidity almost immediately after waking. (9) The sleeper remains alert and responsive toward certain sounds, having a special *meaning* to him. How could a narcotization of the brain have such a discriminating effect upon the mental reactions? (10) The form of the curve representing the depth of sleep at different hours of the night does not conform to that which we should expect if we had to do simply with recovery from fatigue or the elimination of poisons. (11) Diverse biological considerations: Why are some animals, *e. g.,* the cat, able to sleep at any time of night or day? Why do birds sleep for so short a time, especially in view of such prolonged activity? Why do some other animals sleep so very lightly? Why do some sleep regularly in the daytime and others at night? How account for the duration of hibernal sleep? (12) Pathological facts: insomnia of maniacs, who are constantly in movement; case of Siamese twins, who slept in some measure independently of one another, etc.

As a substitute for these various physiological theories the author proposes his own 'biological' theory, *biological* because it regards the subject from the view-point of the total organism. Sleep, he tells us, is not a mere cessation of activity—it is a 'positive function.' This general attitude, he acknowledges, is not original, having been clearly stated by a number of other physiologists and psychologists, *e. g.,* Janet and Myers.

Sleep is not the effect of exhaustion, but forestalls and wards off exhaustion. "It is not because we are poisoned or exhausted that we sleep, but we sleep in order not to be." Thus sleep frequently occurs without there being any other symptoms of fatigue. This lack of proportion between the cause and the effect is what distinguishes a positive activity (reflex or instinctive) from the passive result of a physical or chemical agency. For it is with instinctive and reflex actions, we are told, that sleep must be classed. Specifically, it is an 'instinct of defense.' Thus it is subject to what the author terms 'the law of the interest of the moment,' *i. e.,* the supremacy, in case of conflict, of that instinct which is momentarily of most importance to the organism. This is the explanation, he believes, of various facts which are unintelligible according to current physiological theories. To keep awake, when occupied with important business, is merely an example of the momentary suppression of one instinctive tendency in favor of another.

The discussion of the 'stimuli of sleep' (for sleep, like any other instinctive act, is provoked by stimuli) brings forth some interesting problems. The relation of darkness to sleep would seem at first to be a simple and direct one; and indeed the withdrawal of visual stimuli generally predisposes the organism to sleep. But we have nocturnal animals, which sleep by day, and others, such as cats and dogs, which sleep regardless of the hour of day or night. These cases are all readily intelligible when regarded as adaptations to particular modes of life. The notion of the time of day, or rather of the interval which has elapsed since we last slept, is one of the main factors in determining our going to sleep; and, similarly, as we all know by experience, the duration of our sleep is frequently determined with great exactness. The author cites the case of an orang-outang which slept from six P.M. till six A.M., while in Java, but which, on being borne westward, on its way to Europe, 'gained time' like a chronometer; so that its period of repose was from two P.M. till two A.M. at the Cape of Good Hope!

Hibernal sleep, according to Claparède, is a real sleep, comparable with and historically derived from ordinary sleep. The instinctive character of sleep in general is particularly well exemplified in this phenomenon. Hibernation is not the physiological effect of a lowered temperature, for the latter fails to produce this effect unless the organism is favorably predisposed. This predisposition is largely a matter of the nutritive condition of the organism. Reduced temperature may, however, act as an immediate stimulus to hibernal sleep. The biological significance both of hibernation and æstivation is the fact that the animal is thereby enabled to escape starvation during a period of inadequate food-supply. In this respect it is comparable with the migration of birds. Historically, it is 'the consequence of the habit, contracted by certain mammals [animals] of passing the winter in a hiding-place where they have stored their provisions.' The vitiated atmosphere of the place of seclusion may be a contributory factor, but the increase of carbon dioxide in the blood (affirmed by Dubois) is not the primary cause of the phenomenon. Many readers will doubtless be surprised to learn that the poverty-stricken peasants of certain parts of Russia are wont to pass into a hibernal sleep which may last interruptedly for several months (Volkov).

As to the 'mechanism' of sleep, the author accepts the view that it is a phenomenon of inhibition, which he picturesquely terms a 'psychological suicide.' It is a 'reaction of disinterest for the present situation.' It is not, however, a complete suspension of our 'psychism.' The phenomena of *irritability* persist, but the *reactivity* is temporarily annulled. Against the vaso-motor theories of sleep, he remarks: "I do not see how, by a simple vaso-motor mechanism, one would be able to realize the delicate attentional selections which obey the law of the interest of the moment." It may be rejoined, however, that these same 'delicate attentional selections' are equally difficult to understand on the author's theory of inhibition, which is based upon Verworn's conception of the latter as 'the predominance of assimilation over disassimilation.' The mechanism of sleep, whatever it is, must be as complex as the phenomena to be explained.

Strictly speaking, there can be no 'center of sleep,' the author says, for, as in the case of other instinctive actions, many parts of the brain are concerned simultaneously.

The restorative effects of sleep are due, first, to the enforced *repose,* and second, to the increase of the *trophic* processes in general—this in spite of the fact that the activity of the viscera is lowered.

The author's assignment of a specific 'function' to *dreams,* especially a function so far removed from racial self-preservation as the conservation of our early memories and the exercise of our unbridled fancies, is calculated to startle any one who has followed the trend of recent evolutionary speculation.

Historically, Claparède regards sleep as an acquired function, a contingent rather than an essential phenomenon in the living world. Few comparative studies have been made, but so far no indications of sleep have been found in the Protozoa, and even in some birds it is almost lacking. Indeed, there is no *a priori* reason for assuming the universality of sleep. It is necessary, in a given case, either (1) if the organism expends itself more rapidly than it repairs itself; or (2) if the toxic waste products are eliminated less rapidly than they are formed. Neither of these conditions is a physiological necessity, however.

The author offers two hypotheses as to the phyletic origin of sleep, but he does not commit himself definitely to either view:

1. Sleep was primarily a phenomenon of exhaustion, but became, secondarily, an active inhibition, through the operation of natural selection.

2. Sleep owes its origin to some other need, but the instinct was brought to its present state of development in consequence of its reparative value (here again through natural selection). Its original function

was perhaps the escape of nocturnal enemies. A comparison is here made with the familiar instinct of feigning death.

A comparison is likewise made between sleep and hysteria, the latter condition being characterized as 'an abnormal exaggeration of the reactions of defensive mental inhibition.'

A 'biological' theory of sleep (using the former word, as Claparède has done, in its narrower sense) obviously does not stand in any necessary contradiction to a purely physiological theory. Nor does Claparède make this contention. The phenomenon which is interpreted by him as an instinctive act of defensive inhibition might well have, as its proximate cause, a condition of cerebral anæmia or of hyperæmia, or the disseveration of certain neurones, or any one of the numerous other causes which have been offered in explanation of sleep. Each of these hypotheses must be disposed of on its own merits, as indeed the author has endeavored to do. A complete explanation of sleep will, however, take into consideration its physiological and chemical concomitants as well as its 'biological' significance. In the meantime Claparède has done a service by his attempt to affiliate sleep with various other biological phenomena, even though these latter are themselves far from being well understood.

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A Motor Theory of Rhythm and Discrete Succession. R. H. STETSON. *Psychological Review,* July–September, 1905. Pp. 250–270, 293–350.

The experience of rhythm in music, verse, dancing, waving a baton, or listening to a series of uniform sounds like the ticks of a clock, is explained by the subject carrying on continuous rhythmic movements. This is the extreme form of the motor theory which Stetson adopts. Other advocates of the theory have allowed some place to mental images, to tension without movement, or to organic rhythms of the circulation or nervous system. These are specifically excluded by Stetson. The paper is an elaboration of brief suggestions published in 'Rhythm and Rhyme' (Monograph Supplement of *Psychological Review,* IV., 453–466). The main purpose is to apply the theory to definite rhythms. The experimental data consist of records and measurements of voluntary rhythmic movements of the baton, fingers, and foot while listening to a sound-series or unaccompanied. The paper tacitly assumes that the movements voluntarily made will represent the conditions in the perception of rhythm when the movements are involuntary. My study of the latter makes me hesitate to accept such a conclusion. How far volition changes the involuntary group-movement remains an important question.

The analysis of the voluntary rhythmic movement-cycle is admirably done and aids materially in applying the motor theory. The movement-cycle corresponding to the rhythmic group consists primarily of a beat-stroke and a back-stroke. It is usually complicated by auxiliary movements of smaller muscles which provide the subordinate elements in the group. The action of the muscles is of the ballistic type described by Richer; one set of muscles contracts while its opposing set relaxes.